

## LETTERS TO THE EDITOR.

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## The Condensation of Helium.

IN addition to my short note printed in last week's NATURE (p. 559), let me begin by remarking that as recently as last year, in an address to the Dutch Congress of Natural Science and Medicine, I expressed the opinion that it would be scarcely possible to liquefy helium. Olszewski, from his expansion experiments, had deduced that the critical temperature of helium was lower than  $2^{\circ}$  K. Dewar had no more succeeded in liquefying it by expansion, and some experiences of my own on helium gas sinking in liquid hydrogen seemed to indicate that helium was nearly a perfect gas. At the same meeting I indicated the determination of the isothermals of helium, an investigation with which I was occupied, and which I had prepared by a series of researches, as the direct way to the calculation of the critical temperature.

The first results I obtained with the isothermals changed totally my views on the liquefaction of helium. From the isothermals down to  $-217^{\circ}$ , it followed that the critical point of helium is at nearly  $5^{\circ}$  K., more in harmony with the estimate of the boiling point at  $5^{\circ}$  or  $6^{\circ}$  K. by Dewar, according to the helium absorbed in charcoal, and the determinations at  $-252^{\circ}$  C. and  $-259^{\circ}$  C. confirmed the result. It thence followed that it would be possible, by rapid expansion of helium compressed at 100 atmospheres at the melting point of hydrogen, to pass below the critical temperature, and to cause a mist to appear in the gas. Also liquefaction by the Joule-Kelvin effect seemed possible. It was to put the first conclusion to the test that I made my recent experiments.

The new features of my application of the expansion method to helium were:—(1) the great quantity of gas; (2) the application of a stop-cock on the tube to let off the gas from the tube into a gas-holder, a gas-bag, or a vacuum; (3) an extremely thin-walled beaker, placed in the thick-walled tube to protect the cooled gas against heat conduction. These devices had been used by Olszewski in his experiments on the expansion of hydrogen.

At the expansion a dense cloud appeared, from which solid masses separated out, floating in the gaseous helium, resembling partly cotton-wool, partly also denser masses, as if floating in a syrupy liquid, adhering to the walls and sliding downward, while at the same time vanishing rapidly (20 seconds). There was no trace of melting.

So far as I could judge, then, from my experiments, I considered it probable that this solid substance was helium. The helium had been burned with copper oxide and passed over charcoal at the temperature of boiling hydrogen, and I trusted to have a gas with only very small admixtures. If helium passed immediately to the solid state, then the position of the vapour-line to the adiabatics would be more favourable for condensation than if it passed into the liquid state, and the voluminous aspect of the solid mass was in harmony with this. By the above, and also by other observations, which afterwards gave rise to doubt or proved incorrect, I was for some time under the impression that I had seen solid helium rapidly giving off vapours of the pressure shown by the gas (once more than 15 atmospheres was shown). The continuation of my experiments has shown that they must be explained in quite a different way. By a not sufficiently explained cause, the gas proved to be not so pure as was supposed, considering the method of purification. In analysing what was absorbed by charcoal at the temperature of boiling hydrogen until the charcoal removed no more hydrogen, so that the gas could only contain traces of hydrogen, it could be proved that in one case the gas had contained only 0.45 and in another only 0.37 volume per cent. of hydrogen at most. (About a small possible quantity of neon I could not yet be certain.) But this small admixture must have had a very great influence; for at a first repetition of the experiment with the helium subjected to this new treatment no cloud at all was observed. In this

experiment the velocity of expansion had been too small. At a second repetition with the same gas, but with greater velocity of expansion, a thin cloud appeared and vanished extremely rapidly (1 second). The mist now had a different aspect.

The explanation of the previous observation is to be found in solution phenomena of solid hydrogen in gaseous helium. The phenomena which made the impression of being the giving off of vapour had been the solution of deposited solid hydrogen in the gaseous helium, the latter rapidly returning from the lower temperature to that of melting hydrogen, and the pressure increasing in consequence. Helium at the temperatures that come into account here can, according to the theory of mixtures, take up at every temperature a percentage of hydrogen, determined by that temperature in such a way that it is not deposited at any pressure. With acceptable suppositions one can deduce that at temperatures above the melting point of hydrogen this percentage can be considerable, and that at this melting point itself it can be more than 1 per cent. From mixtures with smaller percentage, the hydrogen is only deposited at lower temperatures, e.g. by expansion. By the smallness of the quantity of hydrogen present it is also explained that, after prolonged blowing off of the helium, no solid hydrogen was left, for the quantity was so small that it could evaporate in the space which it found at its disposal. It is remarkable that so small a quantity of admixture as the gas contained has been able to give the total phenomenon of a substance condensing to a solid and re-evaporating, though the rapid evaporation is in harmony with the smallness of this quantity of substance, considering that even denser masses were seen to be blown away sometimes. There cannot have been much more than 1 milligram or 15 cubic millimetres of solid hydrogen in round numbers in the tube—probably there was less in it—and yet the tube of nearly 7 cubic centimetres was over its whole length for almost a quarter filled with dense, flaky substance.

So far as the experiments on the expansion of helium are now advanced, they show the curious forms that the solution phenomena of a solid in a gas take in the case of helium and hydrogen. They further point to the possibility of realising with mixtures of hydrogen and helium the rising or falling of the solid substance according to the pressure exerted on the gas, the barotropic phenomenon for a solid and a gas. But the question of condensing helium is to be considered yet as an open one.

Let me add a few words as to the mist observed in the repetition of the expansion experiment with the "coal-pure" gas. It is certain that this gas only contains very small quantities of hydrogen. The spectroscopic test also gives traces only. It is possible that the amount of the traces will prove sufficient to attribute the mist to the traces of hydrogen left in the gas. But it is also possible that the mist has been a liquid cloud, and the changed aspect seemed to point to this. If this might prove to be the case, then the critical point would be nearly what I calculated from the isotherms, and helium would obey tolerably well the laws of van der Waals. The tube broke, and so I could not attain more certainty about the nature of the cloud.

The preceding experiments show very strikingly how careful one has to be in arriving at conclusions from the appearance or non-appearance of a cloud by expansion. A decision about the critical point of helium is therefore only to be obtained by a prolonged systematic investigation, which will take much time.

April 14.

H. KAMERLINGH ONNES.

## Satellites of Yellow and Green Lines of Mercury.

BEING engaged with the investigation of the Zeeman effect by using a 35-plate Echelon spectroscope constructed by Hilger, I made an experimental test of the resolving power of the instrument on the yellow and green lines of mercury. With a lamp of the Aron type (30 volts, 6 amperes), and by eye observation with a micrometer, I found the following satellites, some of which seem to be new.  $\delta\lambda$  is given in Ångström units. The measurements by Janicki with an Echelon spectroscope, and by Baeyer



with Lummer-Gehrcke plates, are cited for the sake of comparison :—

$\lambda = 5790$ (Yellow line).				
Observed $\delta\lambda$	Intensity	Janicki	Baeyer	
-0'266	...	-0'251	...	
-0'170	...	-0'187	...	-0'19
-0'122	...	-0'119	...	-0'127
-0'077*	...	—	...	—
-0'032*	...	—	...	—
Principal Line		Principal Line	Principal Line	
+0'035*	...	—	...	—
+0'073	...	+0'084	...	—
+0'142	...	+0'132	...	+0'139
+0'189	...	+0'168	...	—
+0'235	...	+0'230	...	+0'237

$\lambda = 5461$ (Green line).				
Observed $\delta\lambda$	Intensity	Janicki	Baeyer	
-0'247	...	-0'232	...	-0'250
-0'216*	...	—	...	—
-0'175*	...	—	...	—
-0'142*	...	—	...	—
-0'110	...	-0'099	...	-0'107
-0'084	...	—	...	-0'072
-0'058	...	-0'066	...	-0'051
-0'024	...	—	...	-0'025
Principal Line		Principal Line	Principal Line	
+0'033*	...	—	...	—
+0'068*	...	+0'088	...	+0'087
+0'109*	...	—	...	—
+0'143	...	+0'133	...	+0'132
+0'201*	...	—	...	—
+0'230	...	—	...	+0'222

Some of the lines not observed by Janicki and Baeyer, and marked with an asterisk, seem to be new, but the scanty literature on spectroscopy at my disposal does not permit me to conclude which of them were observed for the first time.

Of the numerous satellites of the green line, -0'232, observed by Janicki, is separated into two lines, -0'247 and -0'216, and -0'099 into two, -0'110 and -0'084. The lines -0'216 and +0'033 are evidently the same as -0'208 and +0'032 given by Gray and Stewart. The satellite +0'087 observed by Baeyer, +0'088 by Janicki, -0'093 by Gehrcke and Baeyer, +0'082 by Fabry and Perot, and +0'084 by Houston is resolved into two components of nearly equal intensity, +0'068 and +0'109. Gray and Stewart give only +0'067. The green line was separated into twenty-one components by Lummer and Gehrcke with a single interference plate; here it is separated into fifteen lines. Some of these lines will be separated into components by increasing the resolving power.

The spectrum produced by heating an ordinary vacuum tube of H-shape containing a few drops of mercury, and excited by an induction coil, gave results almost coinciding with those of Janicki, as observed by Mr. Amano and myself. The appearance of the satellites seems to be influenced greatly by the construction of the tube and the mode of excitement.

H. NAGAOKA.

Physical Institute, Tokyo University, March 15.

#### Mendelian Characters among Shorthorns.

I was much interested in Prof. James Wilson's letter in NATURE of April 2, and I sent the number to my friend, Mr. William Duthie, of Collynie, Tarves, Aberdeenshire, a well-known breeder of Shorthorns of the first class, in the hope that Mr. Duthie, from his own experience, might check some of the numbers given by Prof. Wilson. Mr. Duthie sent my note to Dr. Thomas F. Jamieson, of Ellon, who is also a famous breeder of Shorthorns, as well as an agricultural chemist of repute. Dr. Jamieson wrote to Mr. Duthie, and I have the authority of both to send the following extract from his letter, which will interest, not only Prof. Wilson, but also those who may be collecting statistics regarding the Mendelian aspects of the problem of heredity :—

"I have long been of opinion that the Shorthorns have arisen from a combination of a red breed and a white one.

There is a remarkable tendency in them to produce animals which are *entirely* white (unless, perhaps, the ears), more so, I think, than those which are entirely red, and I find that of the white calves the majority are females. I would like you to test this latter point from your own knowledge, in order to see if you also find it so. There is no doubt that a red bull mated with a red cow will almost always produce a red calf, more especially if the bull's own parents were both red, and similarly with white upon white. My red bull "Topsman," 63,447, gave me 113 calves, and not one of them white. He was mated eleven times with a white cow, and the result was ten roans and one red. He was mated sixty times with a red or red-and-white cow, and *every one* of the calves was red. He was mated forty-two times with a roan cow, with the result that twenty-three of the calves were roan and nineteen red. "Topsman" had white socks on the hind shanks, and several of his calves had so too, probably about twenty-six of them, or 23 per cent."

JOHN G. MCKENDRICK.

Maxieburn, Stonehaven, April 11.

#### Ionisation of Air by Ultra-violet Light.

SINCE Lenard has shown that ionisation of the air is produced by light of short wave-length, it has seemed advisable to extend his researches into the region of the extreme ultra-violet, discovered by Schumann, and to investigate the effect on air of light of wave-length below  $\lambda$  1850.

For this purpose, a discharge tube filled with hydrogen to a pressure of 1 mm. of mercury, and a screen-cell, were used, both similar to those described by Prof. Lyman in the *Astrophysical Journal*, March. Below the screen-cell was a chamber where ionisation took place. Dry, dust-free air was blown through this chamber into a cylindrical condenser system. The ionisation produced by the light from the vacuum tube was measured by the charge acquired by one of the cylinders, the other being kept at a constant potential. The air pressure in the screen-cell could be varied at will. Precautions were taken to guard against surface effects.

Under these conditions, it has been found that the ionisation increases in a most marked degree as the pressure in the screen-cell is decreased. It is, therefore, evident that ionisation is produced in air by light from that part of the spectrum discovered by Schumann, and that the effect increases considerably with decrease in wave-length, at all events, in the neighbourhood of  $\lambda$  1800.

It is proposed to investigate the effect in some of the elementary gases.

FREDERIC PALMER, jun.

Haverford College, Haverford, Pa., U.S.A.,

April 10.

#### THE INTERNATIONAL MATHEMATICAL CONGRESS AT ROME.

THE congress of 1908 has been considerably larger than its three predecessors. Up to April 4, the official membership list contained 648 names, but later additions have increased the number of those present to about 530 members, and 167 ladies accompanying them. The weather has been of the same unsettled character that we are accustomed to describe as "British Association weather," but the brilliancy of the gatherings has not been materially affected by the spells of rain.

The proceedings commenced with a reception at the University, given by the rector (Prof. Tonelli) on Sunday, April 5, but the congress was formally opened on the following morning in the Hall of the Horatii and Curatii at the Capitol, in the presence of the King, when addresses were read by Mr. Nathan, Mayor of Rome, by Prof. Blaserna, representing the Reale Accademia dei Lincei, and by the Minister of Public Instruction.

A discourse was afterwards read by Prof. Vito Volterra on mathematical progress in Italy during the